

The method of the reduction is sufficiently explained by the first and second columns of the above specimen computation.

The numbers 601.643 and 579.682 that appear in the second column are the numbers of sidereal and lunar seconds respectively in 10 solar minutes.

The formulæ are easily proved ; an investigation of the Airy correction is given below.

For the figure of the Earth a compression  $\frac{1}{300}$  is used, and  $\rho$  is corrected for the height of the observatory above the sea-level.

The Airy correction is the excess of

$$-\sin^{-1} [\sin p(\rho \sin z + \mu)],$$

the true correction for parallax and semidiameter, over

$$-p\rho \sin z - \sin^{-1} (\mu \sin p),$$

which have been applied under the names of "approximate correction for parallax" and "semidiameter."  $\mu$  here denotes the ratio of the Moon's diameter to the Earth's equatorial diameter, and must be considered negative for the upper limb. Its numerical value is 0.273. The formula is easily reduced to the form

$$\frac{206265''}{6} \sin^3 p \sin z [0.78 - \sin z (\pm 0.82 + \sin z)]$$

for the lower and upper limbs respectively.

### *On the Accuracy of Photographic Measures : Second Note.*

By H. C. Plummer, M.A.

1. Since the appearance of my first note on this subject (*Monthly Notices*, vol. lxi., p. 618), in which I took occasion to examine a recent memoir by M. Lœwy, the discussion has been shared by Mr. A. R. Hinks (*Monthly Notices*, vol. lxii., p. 132). More recently M. Lœwy has published two further memoirs of considerable length (*Conférence Astrographique Internationale, Circulaire No. 9*), in which he has developed his researches in a very elaborate manner. In what follows it will be convenient to denote M. Lœwy's first memoir (*Circulaire No. 8*) and the two later memoirs by  $L_1$ ,  $L_2$ , and  $L_3$  respectively, Mr. Hinks' paper by  $H_1$ , and my own by  $P_1$ . M. Lœwy's purpose, expressed shortly, has been to construct a formula which shall give a superior limit to the probable error of a coordinate determined photographically under any circumstances. If the validity of such a formula once determined be admitted, it will be possible to deduce from it, as M. Lœwy has done, a number of practical conclusions for guidance in the conduct of actual operations. On

the other hand, it may prove more expedient to examine the practical problems which arise individually and in a more simple and direct manner. The possibility of proceeding in this way constitutes one of the vital advantages of the photographic method as compared with methods more familiar in the past, wherein the observations, once made, could not be reconsidered or repeated.

2. The principal conclusions at which I had arrived were not disputed by Mr. Hinks, who, in a desire to avoid reference to the usual theory of errors, employed a line of argument of which it seems difficult to assess the precise value. It may be pointed out, however, that even if all that involved the language of the ordinary theory of errors had been suppressed in my paper the comparative table of discordances (*P*<sub>1</sub>, p. 624) would suffice to prove the main point in question. But there is one small point, in no way affecting my conclusions, on which Mr. Hinks has expressed himself at variance with me. He says (*H*<sub>1</sub>, p. 136), "I do not therefore find myself in agreement with Mr. Plummer when he says: 'We may follow M. Lœwy in denoting the accidental and systematic errors by  $E_a$  and  $E_s$  respectively, and the total probable error by  $E_t$ , and admit that  $E_t^2 = E_a^2 + E_s^2$ .' This is indeed in obvious contradiction to his conclusion that there is a striking compensation of errors when the plate is measured in reversed positions. . . ." I believe that this criticism is due simply to a slight misconception of the policy underlying my paper. In § 5 (*P*<sub>1</sub>), where the words quoted by Mr. Hinks occur, I was following M. Lœwy's views as closely as possible. There was no proof at that stage that M. Lœwy was essentially wrong. Afterwards, in its logical place, the application of the law is denied in sufficiently clear terms. Thus in § 11 (*P*<sub>1</sub>, p. 625) ". . . in general it" (*i.e.* the error removed by reversing the plate) "will not obey the axioms on which the ordinary theory of errors is founded," and again in § 12 (*P*<sub>1</sub>, p. 626): "Evidently we must regard as the unit of measurement, *not* a measure made in a single orientation, *but* the mean of two effected on the plate in the direct and reversed position." These statements sufficiently cover § 5 and remove my responsibility for any preliminary statement made there.

3. But though this explains the point raised by Mr. Hinks, and shows that we are really in agreement, the point may usefully be considered further, and its examination will serve as an introduction to the remarks to be made on M. Lœwy's papers. If we have an error  $t$  which must be considered as made up of two parts  $a$  and  $s$ , so that  $t = a + s$ , then

$$t^2 = a^2 + 2as + s^2.$$

It follows that if mean values are taken of both sides of the equation, in a purely statistical sense and with an obvious notation,

$$\epsilon_t^2 = \epsilon_a^2 + \epsilon_s^2$$

provided that (1)  $a$  and  $s$  are really independent, and (2) positive and negative values of  $a$  are equally likely to the same amount. The formula does hold then in a certain sense for errors of mean square. But if we deduce a corresponding formula for probable errors on the supposition that the probable error bears a fixed ratio to the error of mean square, the result will be not merely wrong but unmeaning, unless both the errors  $a$  and  $s$  obey certain laws too well known to make it necessary to specify them here. And even if we *define* a probable error as bearing the ratio to the error of mean square of '6745 : 1, which would be consistent with the ordinary theory, it would still remain true that the resulting formula would have no useful meaning, and no useful deduction whatever could be derived from it. Similarly it is easy to assume a more extended formula, such as

$$\epsilon_i^2 = \frac{\epsilon_1^2}{m_1} + \frac{\epsilon_2^2}{m_2} + \frac{\epsilon_3^2}{m_3} + \dots$$

when a number of sources of error are in question, and there is no formal difficulty in deriving a set of constants from a sufficient number of equations which are to be obtained by varying the conditions of observation. But, again, it is quite obvious that the result can bear no useful meaning unless we are prepared to subscribe to certain articles of belief as to the nature of the sources of error involved. And there is a further difficulty in practice. The probable errors in the several cases can be determined easily enough, but we can seldom ascribe to them a high degree of precision, and that for several reasons, such as the finite number of the residuals on which they are based. Consequently the more complicated the formula assumed the more will errors accumulate in the determination of the constants, until from this cause alone an absolutely fallacious result will be obtained.

4. The principles here laid down will probably not be disputed : on the contrary, they are more likely to be considered too obvious to need expression. But they are precisely those which the method of M. Lœwy's first memoir disregarded ; and in his later work the same faulty method is pushed to what appear to me extravagant lengths. Whenever a discrepancy arises in the application of the formula it is fatally easy to assume a fresh source of error and to add a term  $\epsilon_r^2$  multiplied by some arbitrary numerical factor. But it is difficult to be assured that the final formula is really in accordance with the laws of probability, or that it is not vitiated by a mass of accumulated error. In the absence of such assurance the results cannot command our confidence. M. Lœwy has assumed the possibility of constructing a formula which shall include all possible sources of error and meet all cases. To speak shortly, I am not prepared to admit that possibility. A too complicated formula of the kind in question seems to me to fall of its own weight.

5. My previous paper was confined to a consideration of the accuracy of the measures of photographic images, and was not concerned with the accuracy of the positions of the images themselves. Although M. Lœwy's later work is devoted to the investigation of the precision of measured coordinates in the wider sense, and to the examination of a number of important questions, I do not intend to enlarge the area of discussion. Mr. Hinks seems to go rather too far when he says (*H.*, p. 137): "It is, then, of no consequence to examine elaborately the divergences from the mean of settings on a single exposure." Undoubtedly this is only a question of a preliminary character, and its solution will not carry us very far. But it is none the less an important one, and appears fundamental in M. Lœwy's method of inquiry. To put the matter as briefly as possible, the main practical conclusion of my former paper was that no essential advantage is to be gained by measuring the position of a single image in more than two orientations of the plate, and that opposite orientations (*e.g.*  $0^\circ$  and  $180^\circ$ ) should be chosen. This contention was opposed to M. Lœwy's view that measures in four orientations were necessary, and was based partly on the fact that M. Lœwy's general formula did not apply to measures made in opposite orientations. From this it appeared that *one class of error* was eliminated by reversing the plate. In an *Avant-propos*, in which he has done me the honour to make reference (but in no sense a reply) to my criticism, M. Lœwy attributes to me the belief that "il suffit, pour tenir compte des erreurs systématiques et pour les éliminer, de faire des mesures dans deux positions de la plaque différentes de  $180^\circ$ ." This rather overstates my view, for I said distinctly (*P.*, p. 626): "I do not mean to say that the whole error, apart from what is purely accidental, can be removed by simply reversing the plate."

6. Before I leave the *Avant-propos*, another passage may be mentioned which seems to indicate a misapprehension in M. Lœwy's mind. Referring to his third memoir, he says: "Cette troisième recherche . . . a . . . révélé ainsi l'existence d'une nouvelle source d'erreur. Contrairement au reproche d'avoir attribué, d'après mes formules, des erreurs probables trop fortes aux résultats, il est arrivé ce qui était à redouter, que cette précision a été surestimée. Cette dernière source d'erreur provient de la déformation de la gélatine. . . . Il résulte de ce fait une inexactitude accidentelle . . . indépendante d'ailleurs de toutes les opérations de mesure." I may be wrong in supposing that this passage is suggested by my "reproche," though the context seems to support that view. But if I am not, it suggests that possibly some slight confusion of ideas may be involved. Clearly it is no answer to the question how accurate are the measures to say that inexactitudes exist which are independent of the process of measurement. As a matter of fact the passage quoted does not apply to my paper at all. My third conclusion (*P.*, p. 628) was: "If M. Lœwy's estimate of the precision

N N

of measures so made be accepted, it does not follow that it is worth while to repeat the measures as many times as he thinks necessary." The grounds on which this opinion was based are set out in § 9 (*P*<sub>1</sub>, p. 623), in which the reason assigned is precisely the "systematic error," *i.e.* "l'inexactitude indépendante de toutes les opérations de mesure."

7. At the beginning of his second memoir M. Lœwy distinguishes two parts in the total error of a measure, the accidental and the systematic, and subdivides the latter into (1)  $\epsilon_{s_1}$  the error which he calls "l'erreur permanente physiologique," and (2)  $\epsilon_{s_2}$ , the error which "provient d'une appréciation erronée du centre du disque." He seems to consider  $\epsilon_{s_1}$  to be of a magnitude too small to enter into the account, an opinion of which something will be said in § 12. With regard to  $\epsilon_{s_2}$  he says (*L*<sub>2</sub>, p. 11): "je considère les deux valeurs de  $\epsilon_{s_2}$  comme étant complètement indépendantes l'une de l'autre aussi bien en signe qu'en grandeur, la personnalité de l'observateur disparaissant ici complètement." In a case when rapid reversal is possible, he admits, however, that "il peut se produire certaines compensations avantageuses d'une partie des erreurs systématiques  $\epsilon_{s_1}$ , si toutefois celles-ci existent." Might it not be suggested that a practical inference, such as that instrumental means ought to be provided for making a rapid reversal possible if the best result is to be obtained, is perhaps of greater value than an elaborate formula based on the principle "qu'il vaut mieux toujours attribuer aux sources d'erreurs leur maximum d'effet."

8. It appears that the second memoir was practically completed before my criticism of the first was published. It is rather hard to say whether the error removed by reversing the plate belongs to the category  $\epsilon_{s_1}$  or  $\epsilon_{s_2}$ , but that matters little. What I hold to be beyond dispute is that in the case of the plate designated *P* by M. Lœwy, a class of error did exist which did not possess the quality of mutual independence, and that his formula broke down when applied in a particular instance. Is it, then, to be supposed that that formula can safely be applied to any plate *except* the very one on which it is based? And this objection is by no means met by M. Lœwy's contentions that the compensations are due to chance. In the case of a series of measures of abscissæ (*L*<sub>2</sub>, p. 77) he obtains a better compensation between certain columns of residuals by making a certain artificial arrangement. It might be suggested that this effect is not wholly unconnected with the fact that the list contains only 21 stars, and these are selected "belles images" within a restricted range of magnitude. But as regards the marked compensations proved in my former paper, I cannot help thinking that M. Lœwy's attempt to attribute them to chance is by no means favourable to his own case. If the disturbing effects of chance are so serious in one case they are likely to be as great in



others, and the numbers which he has deduced from the plate P must all lie under the same suspicion. If the argument be admitted, the weight to be attached to the results of the first two memoirs is small indeed.

9. It may be remarked at this point that M. Lœwy in his first two memoirs has sought to erect a vast edifice on a singularly slender foundation—in fact, on a single plate, if I have not misunderstood him. It is useless in a matter of this kind to seek support in vague appeals to “experience.” On the other hand the strength of the critical position depends on the fact that the objections which have been raised have been based on M. Lœwy’s own material. Now, as to these observations, some comments must be made. M. Lœwy remarks (*L*<sub>2</sub>, p. 11): “En ce qui concerne le cliché P, quelques incertitudes très légères se sont révélées au début des mesures, où les observateurs A et B n’avaient pas la sûreté d’une pratique prolongée, mais ultérieurement, pour tous les deux, l’erreur  $\epsilon_1$  s’est complètement évanouie.” It is unfortunate that a research which pretends to a fundamental character should have been based on the work of inexperienced measurers. Another remark of M. Lœwy’s is also noteworthy (*L*<sub>2</sub>, p. 47): “Je dois à cette occasion faire remarquer que l’observateur B se trouvant absent pendant plusieurs jours, quelques-unes des mesures, huit ou dix séries, ont été effectuées par l’observateur A, substitution qui . . . ne donne lieu à aucun inconvénient.” This is also unfortunate, and reveals the gratuitous nature of M. Lœwy’s assumption that a change of orientation with a single observer is equivalent to a change of observers. It may also help to explain the notable discontinuities which I found in my former paper (*P*<sub>1</sub>, § 7)—and Mr. Hinks confirms me—to exist in the residuals of B in certain orientations. But the fact that measures actually made by A were in effect labelled as those of B stands in no need of comment.

10. One very considerable concession M. Lœwy has made. He has not only given a general formula for the probable error according to his own ideas and assumptions, but he has also given a second solution of the problem which is based on the means of measures effected in opposite orientations of the plate. He admits (*L*<sub>2</sub>, p. 57) that “cette formule aboutit pour  $\epsilon_i^m$ ” (the probable error) “à des valeurs inférieures à celles que donnerait la formule (C).” But he contests its validity on two grounds: “Non seulement la seconde solution ne peut être appliquée aux opérations de mesure exécutées dans une seule orientation, mais elle donne un résultat trop faible et trop éloigné de la vérité.” The latter reason is a mere statement and nothing more. The former objection is irrelevant, for my whole point is that the mean of measures in opposite orientations is to be regarded as the unit of measurement. I do not admit the possibility in general of constructing a single formula which shall represent the accuracy of single measures and those made in

N N 2

properly chosen orientations, because there is no continuity between the two cases. And therefore, when M. Lœwy manipulates his second solution by the addition of numerous terms, there can be no agreement with his method of procedure, nor can the results be accepted of which he says (*L*<sub>2</sub>, p. 59): “Le système de formules (G) ainsi amélioré devient maintenant applicable à tous les cas, même pour les mesures faites dans une seule orientation de la plaque.” This contradicts the very hypothesis (be it right or wrong) on which the formulæ are based, and the results need not be discussed further.

11. Certain points in M. Lœwy's third memoir may now be briefly considered. His object here is to compare the measured coordinates with those deduced from the meridian observations of *étoiles de repère*, and the material for discussion is provided by measures effected on an *Eros* plate designated BI. It is not intended to go beyond the limits imposed in § 5, except to notice one point which presents itself at the end of the memoir. M. Lœwy finds that the probable discordance between abscissæ is 0''.167 when determined by direct comparison and 0''.129 when calculated *a priori* from his formula. He claims that (*L*<sub>3</sub>, p. 115) “la divergence . . . n'est pas en vérité notable.” Now, clearly it might be caused by a systematic error, which has been overlooked, of probable magnitude 0''.10. There is no need to lay any great stress on the discrepancy, which might be accounted for in several ways; though M. Lœwy seems quite inconsistent here when his previous treatment of minute differences is borne in mind. But surely he is not justified in saying: “L'accord entre les erreurs prévues et celles qui se manifestent réellement semble apporter une démonstration décisive de la rigueur des formules destinées à l'évaluation de l'erreur probable des coordonnées rectilignes tirées des clichés.” This expression of unqualified satisfaction does seem somewhat out of place.

12. There is only one other passage in the third memoir which will receive criticism, but that serious criticism. M. Lœwy gives at full length tables which contain the ordinates measured in four orientations by A and B, the differences between the means for the two observers, and the mean of the eight ordinates for each star. It is pleasant to be able to acknowledge gratefully the ample and clear form in which M. Lœwy gives his material. With regard to these tables he says (*L*<sub>3</sub>, p. 88): “A l'aide de ces éléments on peut aisément se convaincre que non seulement les ordonnées trouvées par les deux observateurs sont absolument identiques (abstraction faite des erreurs accidentelles), mais encore que l'erreur physiologique  $\epsilon_{s_i}$  n'existe pas ou n'a pas de valeur appréciable ni pour l'observateur A ni pour l'observateur B.” Comment on the first remark is reserved for § 14. The truth or otherwise of the second statement may be gathered from an inspection of the Table of

Personality which I have constructed by subtracting from each measure the mean for each observer, and adding the results for each orientation and dividing the sums by 43, the number of the stars. The numbers in the first column are those of the sections of Tableau III., corresponding to the six images whose ordinates were measured.

Table of Personality.

Tableau III.	A				B			
	0°.	180°.	90°.	270°.	0°.	180°.	90°.	270°.
2	+ 0.092	- 0.102	+ 0.053	- 0.042	+ 0.070	- 0.037	+ 0.029	- 0.062
3	+ .096	- .117	+ .077	- .056	+ .076	- .044	+ .020	- .050
4	+ .079	- .103	+ .073	- .038	+ .059	- .019	+ .032	- .071
5	+ .092	- .095	+ .071	- .066	+ .086	- .055	+ .050	- .081
6	+ .113	- .112	+ .083	- .085	+ .075	- .046	+ .038	- .068
7	+ .114	- .120	+ .076	- .069	+ .075	- .051	+ .038	- .064
Means =	+ .096	- .108	+ .072	- .059	+ .073	- .042	+ .035	- .066

In the face of these figures can M. Lœwy's denial of the existence of the "erreur physiologique" be maintained? But perhaps he will say they are small. "Valeur appréciable" is a relative term, it is true. But there is an answer to this objection which seems to me final. *The magnitudes of these constant errors are in many cases sufficient practically to control the signs of the residuals.* Thus for the observer A's measures in orientation 0° only 40 residuals are negative in a total of 258! Surely so great an "écart" ought to be attributed to a cause.

13. The task of discussing a set of residuals is always a delicate one, and M. Lœwy has not avoided a serious error. The true principle was expressed admirably by Gauss, and in this matter we have not advanced beyond his position when he wrote: "Scilicet observatoris est, omnes caussas, quæ errores constantes producere valent, sedulo investigare, et vel amovere, vel saltem earum rationem et magnitudinem summo studio perscrutari, ut effectus in quavis observatione determinata assignari, adeoque hæc ab illo liberari possit, quo pacto res eodem redit, ac si error omnino non affuisset." In addition to eliminating the personal equation, there seems to be, contrary to what might have been expected, little extra advantage in the case of these measures in combining opposite orientations. But that is not really surprising. M. Lœwy has detected more pronounced errors in the measures of the fainter stars, and these have been omitted, so that we are dealing only with a limited range of magnitude. The images possess a more homogeneous character, and the measures are naturally of a higher order of accuracy. The "systematic error" with which M. Lœwy alarmed us must be very small, and further compensations cannot be expected when the errors are almost purely accidental. In practice constant per-



sonal equation matters little, for it disappears of course in the process of finding the plate constants. It amounts merely to a virtual displacement of the réseau. But it has one inconvenience. If a doubt arises as to the correctness of the measures of an individual star, it is impossible afterwards to revise the measures with any certainty unless the personality of the original measurer was determined at the time. For instance, this impossibility applies to the whole of the astrophotographic plates measured at the Paris Observatory. I regard some of the smaller numbers in the preceding table as due, not to the absence, but to the variability of the personal equation. The residuals in some cases seem to suggest this idea very strongly.

14. I have already remarked that M. Lœwy's statement that a change of orientation with a single observer is equivalent to a change of observer is simply an unproved assumption. It is interesting, therefore, to examine the differences between the means of measures by A and B of the ordinates of six images of forty-three stars. Now such differences are equally likely to be of either sign, and the chances of 6, 5, 4, 3 like signs are as 1 : 6 : 15 : 10. As a matter of fact, the numbers of sets of differences with the stated numbers of like signs are 7, 11, 11, 14 respectively, where we should expect about 1 or 2, 8, 20, and 13. The numbers are too small to lay any stress upon them. But the number of sets of differences without a change of sign seems rather larger than it ought to be. Accordingly these cases seem to be worthy of being reproduced in detail, an extra star being given which is not strictly qualified to appear. The unit of the differences is 0'0001, and the numbers at the bottom may be regarded either as sums with this unit or as means with the unit 0''001.

Star No.	9	15	17	27	29	36	38	25
Image 1	+ .3	+ 64	- 12	- 9	+ 12	+ 4	- 6	- 30
2	+ 1	+ 57	- 4	- 3	+ 2	+ 12	- 19	- 20
3	+ 2	+ 61	- 10	- 5	+ 80	+ 7	- 7	- 37
4	+ 14	+ 3	- 15	- 32	+ 23	+ 7	- 8	- 19
5	+ 13	+ 45	- 25	- 27	+ 7	+ 4	- 11	- 7
6	+ 8	+ 21	- 26	- 27	+ 11	+ 6	- 14	+ 4
	+ 041	+ 251	- 092	- 103	+ 135	+ 040	- 065	- 109

Now the stars 9, 36, and 38 can be dismissed from consideration as satisfactorily measured. But can the same be said of the others? Each observer has effected his measures in four orientations on six several images. Is it right that in four cases (roughly 10 per cent.) a discordance of practically 0''1 or more should appear between the means, while in a fifth case the discordance actually amounts to a quarter of a second of arc? The odds against such a case as is presented by star 15 must be something like 10,000 to 1, if chance alone is in operation. Of

course it is possible that M. Lœwy has been unfortunate, and has included among his forty-three stars a case which will only recur once in, say, 10,000 times. If that be so, it is singularly unfortunate that all the observations of this star were not most carefully re-examined. It is curious that M. Lœwy nowhere mentions that immense advantage of the photographic over more direct methods, the possibility of easily referring again to the plate in cases of doubt and revising the measures with special care. But here I cannot think that we have to do with a mere play of chance. It does seem that the observers A and B do systematically differ in their estimation of the centre of the star, and as the difference does not appear merely in the measures of a single image a real cause must perhaps be sought in the nature of the star! This will no doubt appear fanciful,\* but it cannot be denied that we have here a singular and apparently inexplicable cause for disquietude. At any rate, it seems clear that the method of multiplied orientations is *not* effective in all cases, though it may be so in the case of the majority of images. If exceptional cases be admitted, we have another reason for saying that no general formula can be constructed for the probable error of a measured coordinate.

15. In conclusion, although M. Lœwy has raised a number of interesting and important questions and has made suggestive contributions towards their solution, the questions have probably not received their final answer, and there seems to be still ample scope for further inquiry. The grounds on which this opinion is based may now be summarised thus :

(a) The general method adopted by M. Lœwy is open to objection for two reasons : (1) It is based on assumptions as to the nature of the errors involved the soundness of which is certainly doubtful ; (2) the results may be vitiated in a great measure by an unavoidable accumulation of arithmetical error (§§ 3, 4).

(b) The observational material discussed is not without defects : (1) It is too restricted in quantity and derived from too small a number of plates ; (2) some of the measures were made by inexperienced observers ; (3) insufficient care was taken to keep the results of different measurers distinct (§ 9).

(c) The contention that personality has no appreciable value cannot in general be maintained. The effect of this equation is proved to be in some cases pronounced (§ 12).

(d) The method of changing the orientations in which measures are effected may serve its purpose in reducing the systematic error of measurement in the majority of cases. But it is at least possible that exceptional cases may occur in which the multiplication of orientations will fail to reconcile estimations of the centre of an image on the part of different observers.

\* Perhaps a more reasonable cause of the systematic discrepancy may be found in local irregularities in the character of the réseau lines.

An instance is quoted in which such a discrepancy persists even for different images of the same star (§ 14).

*University Observatory, Oxford:*  
1902 May 7.

*Visual and Spectroscopic Observations of the Sun-spot group of*  
1901 May 19–June 26. By the Rev. A. L. Cortie, S.J.,  
F.R.A.S.

The group of Sun-spots of 1901 May 19–June 26, seen during two solar rotations, is noteworthy for several reasons. First, it was a large group visible to the unaided eye, occurring at a time of Sun-spot minimum. If one group of small spots seen between March 3–11 be excepted, the solar surface had been quiescent for almost exactly six months before the appearance of this group, without any spots and with very few faculae, and these faculae more than ordinarily faint. The following table deduced from the Stonyhurst series of Sun-spot drawings, supplemented by a very full series of direct visual observations by Mr. Hadden, of Alta, Iowa, U.S.A., shows the quiet state of the solar surface between 1900 November and 1901 April before the advent of the big group of May–June. The greatest visible disc-area of the group from the Stonyhurst drawings was 7·6 on May 22, the unit being  $\frac{1}{5000}$  of the visible disc. In the former Sun-spot minimum of 1889 a big spot also appeared, that of June, as a contrast to the general quiescence. The recurrence of the phenomenon during the present minimum, even if only a coincidence, is noteworthy.

Month.	Days of Observation.	Mean Daily Spots.	Number of Groups. Faculae.
1900 November	26	0·42	0·62
December	29	0·10	0·59
1901 January	28	0·00	0·18
February	26	0·31	0·58
March	30	0·53	0·63
April	30	0·00	0·70
May	31	0·61	1·09
June	30	0·73	0·73

1. A unique interest attaches to the 1901 outburst in that, as Mr. Perrine and the Greenwich observers have shown, it was very close in position when just out of sight beyond the E. limb on May 18, to a fine prominence, and, what is still more remarkable, to a disturbed area of the solar corona observed during the total